Horticultural Society. This cross was between *S. multiflorus* (female) and several colour forms of the "Cultivated Cineraria," embodying certainly two distinct species. No direct evidence on origin has been in this particular experiment obtained, or expected, but the predominating influence of the "Cultivated Cineraria" in this and also in the reverse cross, upon the colour and size of the flower-heads, appears to suggest, I think, a possible predominance of one species over another, in other cases. The crosses to which I have referred, taken together, sufficiently demonstrate an extreme readiness to cross, since every one of the thirteen attempts has resulted in a numerous hybrid progeny, while not one of the several hundreds of plants raised has failed of being a hybrid. The only care taken was to exclude insects, which might have brought pollen from another plant, by means of muslin, and no attention was paid to the pollen produced within the muslin bags. This pollen, on the evidence of nearly five hundred plants, had no effect. From the facility with which these plants cross under cultivation—even the woody *S. Heritieri* with the completely herbaceous kinds—it is likely that they cross also in a state of nature, whenever the opportunity occurs. It would be interesting, therefore, to have information of the relative distribution of the kinds, and to know of all variations. There is no doubt a large field and good motive for exploration in the Canaries, and I should be exceedingly thankful for any seeds or plants that may be sent me.

Botanic Gardens, Cambridge. R. IRWIN LYNCH.

Prichard and Acquired Characters.

PROF. MELDOLA, in his suggestive address to the Entomological Society on January 20, very rightly puts Prichard before Galton and Weismann in the list of those who have formulated the theory that acquired characters are not inherited by offspring. Some years ago, when Platt Ball's interesting little book was published on the question, "Are the Effects of Use and Disuse Inherited?" I was struck by the careful way in which the author pointed out how "so sound and cautious an observer as Francis Galton had also [*i.e.* as well as Weismann], in 1875, concluded that 'acquired modifications are barely, if at all, *inherited* in the correct sense of that word.'" At the same time my memory went back to some allusions I had seen in an old book (Coombe's "Constitution of Man," Edinburgh, 1836, a copy of which I had recently bought at a marine store dealer's for one halfpenny) to the theories of Dr. Prichard. I subsequently looked up the references and made some notes, which have until now been pigeon-holed. I quote three passages and a note as bearing on the question, Who first pointed out that acquired characters are not inherited? ("Researches into the Physical History of Mankind," vol. ii. p. 536, by James Cowles Prichard, M.D., F.R.S.)

"It has often been a question among physiological writers what peculiarities of structure are liable to be transmitted by parents to their offspring, and what terminate with the individual, without affecting the race. Perhaps the following remarks may afford the solution of this difficulty:—

"It appears to be a general fact that all connate varieties of structure or peculiarities which are congenital, or which form a part of the natural constitution, impressed on an individual from his birth, or rather from the commencement of his organisation, whether they happen to descend to him from a long inheritance or to spring up for the first time in his own person, are apt to reappear in his offspring. It may be said, in other words, that the organisation of the offspring is always modelled according to the type of the original structure of the parent.

"*On the other hand, changes produced by external causes in the appearance or constitution of the individual are temporary, and, in general, acquired characters are transient; they terminate with the individual, and have no influence on the progeny.*[1] [The italics are mine.]

Dr. Prichard has very properly mentioned the source of his ideas, and it is to be hoped that we also may give credit where credit is due. WILFRED MARK WEBB.

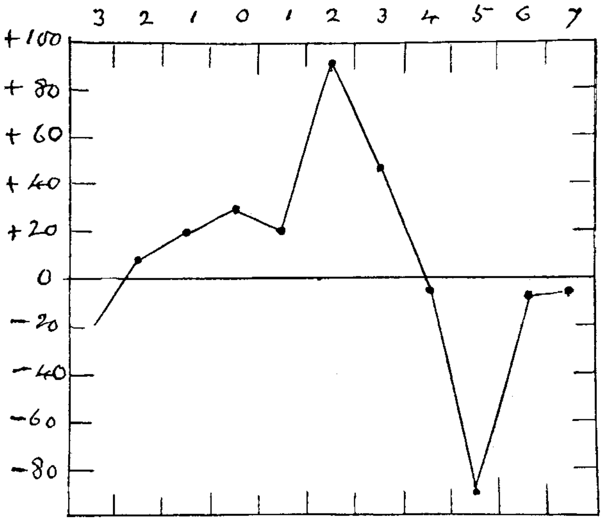
Biological Laboratory of the Essex County Council,
Chelmsford, January 27.

I AM very glad that Mr. Webb has also directed attention to Dr. Prichard's share in the establishment of the doctrine of the non-transmissibility of acquired characters. I should like to add that my attention was first called to the work in question (2nd edition, 1826) by my father-in-law, Dr. Maurice Davis. Prof. Poulton has taken the subject in hand, and is preparing an article on the whole question. R. MELDOLA.

Rainfall in the Lake District.

THE recent publication, in Mr. Symons' *British Rainfall* for 1895, of fifty years' data of rainfall at Seathwaite (1845–94), affords an opportunity of studying the climate of this very wet district in relation to the vexed question of sunspot influence on weather.

The following might, perhaps, be offered for criticism. Consider each maximum sunspot year, three years before it, and seven after it. (The intervals from maximum to minimum, it is known, are generally longer than those from minimum to maximum.) Indicate the character of each year with a + or − sign, according as it is above or below the average (137 in.). We may further give the algebraic sums of each vertical group, and of certain horizontal groups (*a* to *b*) as shown; and plot in a curve the values for the vertical groups. (Should any objection be taken to comparing vertical groups of four members with those of five, I may say that exclusion of the lowest row of values (1893 group) does not materially alter the result.)



Sunsp.			*a*	Max.			*b*					Sums
Max.	3	2	1	0	1	2	3	4	5	6	7	*a* to *b*
1848	+	+	−	+	−	+	+	+	−	+	−	+ 14
1860	−	−	+	+	+	+	+	−	−	+	−	+133
1870	−	+	+	−	−	+	+	+	−	−	+	+ 29
1883	−	−	+	+	−	+	−	−	−	−	+	+ 27
1893	+	+	−	+	+							+ 5
	−21	+9	+20	+30	+20	+92	+46	−5	−88	−6	−5	

These latter values we find rising (with one slight break) to a maximum in the *second* year after the sunspot maximum, then sinking rapidly to a minimum in the fifth year, and continuing under average in the two following years.

It will be noted that in the enclosed groups *a* to *b*, the + signs largely preponderate (16 + to 7 −); that they preponderate in each horizontal group, and in each vertical group but one; and that the sums of all those groups have + signs.

A. B. M.

The Epistemology of Natural Science and Mr. Karl Pearson.

ONLY a few days ago I happened to see the review of my "Erkenntnistheoretische Grundzüge der Naturwissenschaften" (Leipzig, 1896) in NATURE of November 5, 1896. I too highly esteem English science and literature to follow Mr. Karl Pearson in the department of his "familiar ideas"; I shall confine myself to showing how little my reviewer has succeeded in rendering my views (see p. 3).

"[1] This distinction, which has not been pointed out by any former writer on physiological subjects, was first suggested to me in conversation, many years ago, by Mr. Benjamin Grainger, of Derby."

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I have nowhere called the undulatory *theory* of light an hypothesis; I have called the undulatory *conception* (Vorstellung) of light an hypothesis. I have nowhere said that "it is the *sinnliche Wahrnehmung* which changes the *Hypothese* to the *Naturgesetz*." Perception, as was seen in the case of photographs of light-waves obtained by Prof. Wiener, makes hypothesis a matter of fact. To call this fact a law, has never come into my mind.

I have nowhere stated Galilei's law of inertia to be a law of nature; I have declared it to be a postulate laid down on the basis of rich empirical materials (ein auf grundreichen empirischen Materials aufgestelltes Postulat, p. 169). Newton's law of gravitation and the principle of energy are typical examples of natural laws. The law of inertia belongs to the "axiomata sive leges motus" of Newton, and in this respect I have compared the law of inertia to the axioms of geometry, and, considering the rich experience of *to-day*, I have spoken as of an *appearance* of obviousness and immediateness (von einem *Scheine* des Einleuchtenden und Unmittelbaren, p. 168). How little I really think the law of inertia to be so obvious and immediate (so einleuchtend und so unmittelbar), my reviewer might also have learned from pages 74–76 of my book. In a postulate, as well as in a law of nature, immediate perception is entirely out of the question.

These few examples will suffice to show that my reviewer, perhaps owing to some difficulties in understanding the German language, has constructed certain ideas which, in fact, do not exist at all, and are, by no means, advocated by myself. As it is, I do not feel obliged to enter into a discussion of his other remarks, in which he, at least, quotes from my book correctly.

PAUL VOLKMANN.

University of Königsberg, January 18.

FOR our *Principle of Inertia* the Germans use two expressions, *Trägheitsprincip* and *Trägheitsgesetz*. Dr. Volkmann in his work almost invariably adopts the latter, and even speaks of the principle of inertia as *das physikalische Gesetz der Trägheit*. If a physical law be not a law of nature, Dr. Volkmann ought to have carefully distinguished between a *Naturgesetz* and a *physikalisches Gesetz*. I am sorry, however, to have given him more credit for logical consistency than he desires to lay claim to. The law of inertia describes how an insensible particle would move relatively to certain "fixed axes" under certain purely conceptual, not physically realisable conditions. The law of gravitation describes how two insensible particles would move relatively to certain "fixed axes" under certain purely conceptual, not physically realisable conditions. If Dr. Volkmann wishes to draw a distinction between the two, and calls the latter alone a law of nature, then my opinion of his *Erkenntnistheoretische Grundzüge* is now lower than it was when I wrote my notice of it. Dr. Volkmann refers me to pp. 74–76 of his work to illustrate that he did not consider the law of inertia *so einleuchtend und so unmittelbar*, yet those are precisely the pages from which I cited in my review the motion of the railway train, which Dr. Volkmann considers can illustrate the law of inertia, an example which I hold to be most illusory. It is especially dangerous to the young student, and most misleading in popular lectures like Dr. Volkmann's *Allgemein wissenschaftliche Vorträge*.

Dr. Volkmann tells us that Wiener's researches have changed the undulatory conception (*Vorstellung*) of light into *eine vollendete Thatsache*, it has now ceased to be an hypothesis. How a conception can become a physical fact by any amount of research, I fail to understand; although I do grasp how the contents of that conception may by new discoveries be found to adequately describe our physical sensations. But when the contents of the conception are found to adequately describe our sensations, then it seems to me that the orderly account of those contents, which we term theory, ceases to be hypothesis, and becomes a law of nature. If Wiener's work makes the undulatory conception an adequate account of sensation, then it converts the undulatory theory into a law of nature. But Dr. Volkmann tells us this is not what he has said. Perhaps the undulatory theory, now that the undulatory conception has become *eine vollendete Thatsache*, still remains a theory, or is a postulate, or a physical law, or something else in Dr. Volkmann's classification. I certainly do not sufficiently grasp Dr. Volkmann's *Grundzüge* to be able to classify it.

Lastly, I hope my "understanding of the German language" has misled me; otherwise I should say that Dr. Volkmann's present interpretation of what he has written on p. 168 "appears" to me disingenuous. What he has written there runs:—

"Wir befinden uns dem Trägheitsgesetz gegenüber heute vielleicht in ähnlicher Lage wie der Geometer seinen Axiomen gegenüber, dem es gerade darum so schwer fällt, an seinen elementaren Sätzen erkenntnistheoretische Studien anzustellen, weil der Inhalt dieser Sätze so einleuchtend, so unmittelbar zugänglich ist. So scheint dem Physiker heute das Trägheitsgesetz so einleuchtend, so unmittelbar, dass es als Axiom vorgetragen zu werden pflegt. Aber es gab eine Zeit, wo der Inhalt des Trägheitsgesetzes dem menschlichen Geiste durchaus nicht so unmittelbar zugänglich erschien, und dies werden wir uns zu vergegenwärtigen haben, um die Bedeutung der Galileischen Forschung noch heute würdigen zu können."

I take this to mean that the law of inertia appears obvious to the physicist of to-day, but at the time of its discovery it was not at all an obvious conception. Dr. Volkmann says that in this passage he has spoken *von einem Scheine des Einleuchtenden und Unmittelbaren*. He has certainly used the verb *scheinen*, but when I say, for example, that twice two makes four *appears* to me a direct and obvious truth, I certainly do not mean to indicate that the directness and obviousness are *specious*. I must apologise to Dr. Volkmann for having misunderstood this subtlety of the German language.

It is perhaps necessary to add that Dr. Volkmann is raising a verbal controversy which has nothing to do with our radical difference of view. For me a law of nature is purely a product of the human intellect; it is a formula which describes in the briefest terms yet discovered as wide a range as possible of the motions we attribute to atoms, particles, molecules, ether, &c., which kinetic *concepts* form parts of the entirely *conceptual* model by aid of which we describe the sequences of our physical sensations. For Dr. Volkmann the law of nature is something existing *ausser uns* (p. 56) in some manner kept in harmony with the *Denknothwendigkeiten in uns*. Presumably the law lies in the *Dinge an sich*, for it would be impossible to find a law like that of gravitation in the contents of our physical sensations. It is at this point that the older view of the physical sciences, as something quite different from *descriptive* sciences, runs us aground on the metaphysical mudbank.

KARL PEARSON.

Durham Degrees in Science.

JUST two remarks in answer to the Rev. Henry Palin Gurney's letter in your last issue.

He admits by his silence my main contention, viz.—that the nature of the M.Sc. degree at Durham has been radically changed owing to the recent action of the University in granting the degree by vote of Convocation.

I did not insinuate in my letter that "these gentlemen had no qualification for the honour."

It is impossible for me to know the necessary qualifications for the degree other than those published by the University in their Calendar. It was sufficient for me that the latter were ignored.

X.

February 6.

ON THE CONDUCTIVE EFFECT PRODUCED IN AIR BY RÖNTGEN RAYS AND BY ULTRA-VIOLET LIGHT.[1]

WE propose in this communication to describe results of experiments on the electrical effects of Röntgen rays and of ultra-violet light when shone on metals, or through air between two metals mutually insulated; and electrified to begin with, by previously producing a difference of potentials between platinum electrodes of an electrometer metallically connected with them. In some of our experiments this potential-difference was zero, and the initial ± electrifications of the opposed surfaces depended solely on difference of volta-electric quality between their opposed surfaces.

To investigate the effects of Röntgen rays, a hollow cylinder of unpolished aluminium connected to the

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[1] A paper by Lord Kelvin, Dr. J. C. Beattie, and Dr. Smoluchowski de Smolan, read before the Royal Society of Edinburgh, February 1.

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